species might be given merely by the terms *sphærenchyma* and *ovenchyma*.

In both species the sides of the cells are somewhat flattened from mutual pressure; and the intercellular passages are either very narrow or not easily seen when the parts are quite moist.

Fig. 1. Fig. 2.

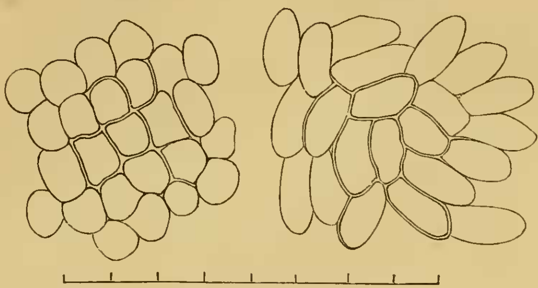

Fig. 1. Outlines of leaf-cells of *H. Tunbridgense*.
Fig. 2. The same of *H. Wilsoni*.

[Both drawn to the one scale of $\frac{1}{500}$th of an inch.]

Edenbridge, July 9, 1863.

XIII.—*On the Value of the Distinctive Characters in* Amœba. By G. C. Wallich, M.D., F.L.S., &c. &c.

In a series of papers published in the 'Annals and Magazine of Natural History' for April, May, and June, 1863, I adverted to the absolute necessity of long-continued and daily observation whensoever it is desired to elucidate the characters and vital phenomena which appertain to the lowest and, at the same time, the most minute forms of organic existence—my remarks on this head having been specially prompted by the truly Protean aspects under which *Amœba villosa* presented itself to my notice.

A fourth month's close study of that form has not only lent additional force to my previous descriptions, but, whilst it enables me to speak with still greater confidence on the subject, it also demonstrates in a striking degree, as I shall presently show, the fallacy of attempting to arrive at a correct knowledge of the characters and ever-varying phases of such an organism under a less laborious and protracted examination.

After the last paper of my series was completed, namely, on the 20th of May, Mr. Carter called on me; and for the first time I made the acquaintance of a naturalist whose researches amongst the lower forms of animal life have always been justly regarded as well worthy of attention. On a subsequent occasion, I endeavoured to exhibit to him, as far as his time permitted, the characters of *Amœba villosa* as then observable in living specimens taken from my aquarium, dwelling strongly, however, on the marked changes which had already taken place in them under the unfavourable conditions of long captivity.

Courting, as I have avowedly done, the fullest scrutiny into the characters and vital phenomena of the Rhizopods alluded to in my descriptions, I confess I was by no means prepared to find that, under an evident misapprehension of my meaning, the view entertained by me throughout my protracted survey of the *Amœba villosa* (namely, that probably many, if not all, of the previously described forms of *Amœba* are referable to, and constitute mere phases of, this the most highly developed type*) should have been adduced in support of the statement that *Amœba villosa* is now regarded by me as identical with *A. princeps* only.

That such has never been my belief may be gathered both from my own account of the first-named species, and from a note appended to the *résumé* of my papers by Mr. H. J. Slack, which appeared in the July Number of the 'Intellectual Observer,' simultaneously with Mr. Carter's notice on *A. princeps* in the 'Annals.'

Much as I regret the necessity of having to become the critic of Mr. Carter's opinions, in order that I may adequately sustain my own, I must state my reasons for declining to subscribe to many of the conclusions at which he has arrived. These reasons will appear whilst I endeavour to establish the four following propositions:—

1. That it is entirely opposed to usage and rule to change the name under which an object shall have been, for the first time, accurately described.

2. That the characters of *Amœba villosa,* as first brought to notice by me in the three published papers to which allusion has been made, are sufficiently important and distinct from those of any previously described form to warrant their being regarded as typical.

3. That certain characters regarded by Mr. Carter as of primary importance, and typical of *A. princeps* (Ehr.) as now *reconstituted* by him, are distinctive of *A. villosa* as already described by me.

4. That the interpretation put by Mr. Carter upon certain other characters which are common to all the *Amœbæ* is negatived by the strongest evidence.

With regard to the first of these propositions, I beg to state

* See note at commencement of my first paper in the 'Annals,' No. 64, for April 1863, p. 287.

at once, and distinctly, that in speaking of the most important and previously undescribed characters of *A. villosa*, I specially allude—(1) To the presence of a villous organ, and the varied phases it assumes as occasion may require. (2) The invariable situation of this organ with reference to the rest of the body, so as to indicate a definite posterior and anterior portion. (3) The well-marked prehensile office of the villi. (4) The extrusion of effete matter through an aperture* in the midst of the villous region. (5) The occasional extrusion of vacuolar vesicles by a similar aperture. (6) The occasional circulatory movement † of the nucleus and contractile vesicle along with the rest of the endogenous as well as exogenous contents of the body. (7) The circumstances under which one or both of the above organs remain, as it were, fixed in the vicinity of the villous region. (8) The discharge, externally, of the contents of the contractile vesicle through an aperture within the same region. (9) The occasional extrusion of perfectly formed minute individuals, also through an aperture in the villous region. (10) The projection of pseudopodia from every portion of the surface except the villous region. (11) The movements always in a direction opposite to the situation of the villous region. (12, and last) The possibility of completely detaching the membranous-walled nucleus from the parent mass by pressure, without laceration or destruction of its wall.

I confidently affirm that none of these characters had been described in any published work whatever, prior to my description of them in the 'Annals' for April, May, and June last.

As regards the first proposition, I may be permitted to observe that, in 1856, I detected an *Amœba* in Lower Bengal, which I am now satisfied was identical with *A. villosa* or a variety of it. This was figured in the first part of my work on the 'North-Atlantic Sea-Bed,' and referred to cursorily in the 'Annals' for April last (p. 290). But, putting this fact out of

* MM. Claparède and Lachmann have stated their belief in the possible existence of an *oral* aperture in *Amœba*, and its actual existence in *Podostoma*. But they have not noticed, as far as I am aware, the occurrence of anything like an excretory orifice always showing itself at one determinate portion of the body.

† In one sense, this character may not be regarded as new, since the permanent relation of the nucleus and contractile vesicle to the rest of the body, whilst moving, has been clearly pointed out in some varieties of *Amœba* by those excellent observers, MM. Claparède and Lachmann. But they appear not to have associated this character with any distinct and permanent differentiation of a determinate portion of the body. What I desire to indicate is the twofold character—these organs at one time remaining fixed near the villous region, at another *not* holding any definite relative position either to any particular portion of the body or to each other.

the question, inasmuch as the Bengal form has not been specially described, in order to render evident the true value attached to all unpublished researches, I need only quote the subjoined extract from the 'Report of the Committee of the British Association for 1842, appointed to consider the rules by which the nomenclature of zoology may be established on a uniform and permanent basis:'—

"Unless a *species* or group is intelligibly defined when the name is given, it cannot be recognized by others, and the signification of the name is consequently lost. Two things are necessary before a zoological term can acquire any authority, viz. *definition* and *publication*. Definition properly implies a distinct exposition of essential characters; and in all cases we conceive this to be indispensable, although some authors maintain that a mere enumeration of the component species, or even of a single type, is sufficient to authenticate a genus. To constitute *publication*, nothing short of the insertion of the above particulars *in a printed book* can be held sufficient." * * * "Nor can any unpublished descriptions, however exact, claim any right of priority till published, and then only from the date of their publication. The same rule applies to cases where groups or species are published, but not defined. Therefore (§ 12) *a name which has never been clearly defined in some published work should be changed for the earliest name by which the object shall have been so defined.*"

Here, then, it will be seen that, independently of the very secondary question as to priority in discovery of the essential characters which I believe indicate the typical *Amœba*, the really important point for determination (namely, the validity or otherwise of the definition assigned by the original founder of the species *A. princeps*, as embracing *A. villosa*) is not left in doubt for a moment. And hence, although the characters so assigned to *A. princeps* must be held as insufficient for the definition of *A. villosa*, those assigned by me to the latter form are such as to embrace every character previously assigned to *A. princeps*. Under these circumstances, I apprehend it is quite unnecessary for me, in view of the rule just quoted, further to discuss the grounds upon which I am reluctantly compelled to object to the course followed by Mr. Carter in making a new definition for *A. princeps* so as to embody the characters of the all-important organ to which attention was directed by me in my late papers. The statement at page 43 of Mr. Carter's paper in the 'Annals' for July, namely, that "the villous appendage which marks the posterior end of *A. princeps* has lately been brought to notice by Dr. Wallich in the species for which he has proposed the designation of *A. villosa*," in the absence of any intimation of the fact that both the discovery and the name are mine, coupled with the remark which immediately follows as to having figured the villous appendage in his "Indian Journal" * so far back as

* Of course I am writing under the impression that by the term "Indian Journal" is meant a private and unpublished journal.

1854, and the declaration, at the commencement of the paper, that the same nomenclature should be adopted as he had used in papers published in 1856, necessitates the inference that I had adopted his name without the usual and due acknowledgment. Whilst distinctly stating that neither of these conclusions is reconcilable with the facts of the case, I would express my conviction that, as Mr. Carter could not intentionally have conveyed such an impression, he will be the foremost to eradicate it, more especially when I point out that any one unacquainted with my notices in the 'Annals' for April, May, and June last, perusing his paper of July, could not fail to regard every one of the characters peculiar to these *Amœbæ* as having been brought to notice for the first time by Mr. Carter. Moreover it will be seen, on a careful comparison of our respective statements, that, although he does not indicate whose opinions he is endeavouring to controvert*, the previously unknown nature of the characters conclusively point to mine.

Although satisfied from Mr. Carter's statement in the last Number of the 'Annals,' that the *Amœba* met with by him in Bombay, in 1854, was in all probability the same form he now describes under the name of *A. princeps*, it is very certain, from what he wrote regarding the typical characters of *Amœba* in 1856 and 1857 (due reference to which will be made hereafter), that he did not regard them as sufficiently distinct to demand special notice; otherwise it is difficult to conceive how he failed to furnish a record of them in any of his published papers from 1854 to July 1863. For it is necessary to mention that, in a paper by Mr. Carter, entitled "Notes and Corrections on the Organization of Infusoria, &c.," which appeared so recently as the year 1861 (Ann. Nat. Hist. ser. 3. vol. viii. p. 281), no reference was made to any modification of his opinions on the points now at issue.

Having thus stated my reasons for thinking it would have been but just had Mr. Carter adduced under the same specific designation whatever further information he possessed concerning an organism which cannot be regarded as distinct from *Amœba villosa*, it is due to myself to state explicitly (with reference to my declaration that many of the so-called species of *Amœba*, if not all of them, are referable to a single specific type) that although of opinion that *A. bilimbosa*, *A. radiosa*, *A. princeps*, *A. Rœselii*, and other varieties are nothing more than imperfectly developed phases of *A. villosa*, inasmuch as none of the striking characters pointed out by me as appertaining to *A. villosa* had been indicated in any of the published defini-

* Had Mr. Carter done so, the necessity for these observations would have been altogether spared me.

tions of these forms, and there were no grounds for assuming that one of these varieties approached more nearly to it than the rest, I had no alternative but to designate my new form by a new specific name—leaving it to be determined hereafter whether *Amœba princeps* and those other forms which have received distinct specific appellations, on trivial differences in their configuration, are or are not mere transitional phases of the most highly developed type, namely, *A. villosa* *.

Fortunately the means of verifying or refuting every statement advanced by me with regard to *A. villosa*, both in matters of fact and deduction, are at hand, and will become more and more abundant as soon as favourable habitats for this Rhizopod shall be discovered. But should any lingering doubt remain as to the impropriety of altering the original definition of *A. princeps*, in order to render it conformable with the characters observable in *A. villosa*, it is only necessary to refer to Mr. Carter's declaration † that "Ehrenberg's ‡ and Dujardin's § figures are good representations of" *A. princeps*, and to beg the reader to examine the plates and definitions here alluded to. On doing so, he will find that neither in the figures themselves nor in the letter-press definition accompanying them is reference made to a single character on which I have based the typical stability of *A. villosa* ||.

* The following extract from Mr. Carter's supplementary paper on "the Infusoria of Bombay," published in 1857, will show that, whilst he is fully alive to the necessity of re-naming an imperfectly defined form, he has put the principle into practice on differences of structure which bear no comparison, in point of importance, with those now assigned to *Amœba villosa*:—"*Euglypha pleurostoma* is very like Ehrenberg's *Difflugia Enchelys* and Dujardin's *Trinema acinus*; but not being identical with the *figure* given of the former, and though often presenting three radiated prolongations like the latter, but by no means so constantly, it becomes necessary to give it a name." (Ann. Nat. Hist. ser. 2. vol. xx. p. 35.)

† Ann. Nat. Hist. July 1863, p. 31.

‡ Infusionsthierchen, Atlas, fol., tab. 8. fig. 10 (1838).

§ Hist. Nat. des Zoophytes, Atlas, plate 1. fig. 11.

|| I subjoin the definitions in question:—"*A. princeps. A.* major dilute flavicans, sextam lineæ partem repens, processibus variabilibus, numerosis, cylindricis, crassis, et apice rotundatis." (Ehrenberg's Infusionsthierchen, p. 126.)

"*A. princeps*, majeure. Large de 0·37 à 0·60, blanc jaunâtre. Remplie de granules qui refractent la lumière, et se portent ou refluent dans les expansions successivement formées, lesquelles sont très-diaphanes à l'extrémité et souvent très-longues" (Dujardin, Hist. Nat. des Zoophytes, 1841, Paris); whilst appended to the plate is the subjoined remark, distinctly indicating that, irrespectively of the granules, there was nothing to show the direction in which the animal might be moving:—"Elle est avancée à la fois ses deux branches en y poussent la substance glutineuse dont elle est formée avec les granules nombreux et variés qui s'y trouvent engagés et qui montrent bien la direction du mouvement" (*loc. cit.*).

Mr. Carter, at page 37 of his paper, says, "Now, the worst of theories is, that they take up so much time in discussion before they bring out fact; while the best of them is, when multiple, that they prove that the fact is still unknown." Again, at page 38, "Unless we can state in a few words the facts we may wish to establish, it is useless to have recourse to long argumentative theories for this purpose,"—the first remark following immediately on Mr. Carter's reference to my view regarding the reciprocal convertibility of the *ectosarc* and *endosarc*—not of "diaphane" and "sarcode," as he, no doubt inadvertently, puts it.

These remarks may be true in the abstract; but it will, I think, be allowed that, in describing objects visible only under the microscope, theories are unavoidable, inasmuch as the determination of the appearances and offices of each part depends more or less on interpretation. In non-microscopic objects, differences of interpretation as to actual appearances can rarely take place, whatever may be the case as regards deductions based on them. But emanating as these strictures do from an author whose writings are so singularly fertile in speculative physiology, they might perhaps advantageously have been avoided, more especially since I do not advance my view touching the reciprocal convertibility of endosarc and ectosarc as a bare speculation, but as a theory supported by evidence so strong that I have little doubt it will be very generally accepted.

Having, for the present, disposed of the question involving a principle of scientific nomenclature, I would request attention to matters of actual observation. And, in order to facilitate reference, it shall be my endeavour to comment on the various subjects, as far as possible, in the order in which they are treated in Mr. Carter's recent paper.

After stating that he met with *Amœba princeps* in April 1863, and his intention of applying to it specially the nomenclature proposed by him in his "Notes on the Organization of the Infusoria of the Island of Bombay" (1856), Mr. Carter says,

"The most conspicuous features of *A. princeps*, when it is large, are its size and the number of granules it contains, in both of which characters it much exceeds any other *Amœba* with which I am acquainted. Its form, of course subject to Protean changes, is for the most part limaceous, or once or twice branched; and its pseudopodia, which are almost always lobed and obtuse, *proceed from a posterior end which is normally capped with a tuft of villous prolongations*; while the *distinguishing character of the nucleus* .... consists in the nucleolus being so much extended," &c. &c., as to cause "the pellucid halo which is seen round the nucleus of other *Amœbæ* to be absent."

If the usual practice of stating specific characters in the order of their importance can be taken as a criterion of their value, the villous organ must evidently be regarded as holding no very prominent position in the form under definition. But although this organ is noticed in the leading "specific description" given by Mr. Carter of *A. princeps*, it is altogether omitted in the enumeration of "the parts of which that form is composed," to which attention is drawn immediately afterwards *; whilst the description of this most essential organ, deferred almost to the close of that portion of the paper which treats of *A. princeps*, and until the general features common to all *Amœbæ* have been discussed (Annals, July, p. 43), is introduced under the head "*Villous appendage*," with the subjoined remark :—

"The villous appendage which marks the posterior end of *A. princeps* has lately been brought into notice by Dr. Wallich, in the species for which he has proposed the designation of *A. villosa*," it being immediately afterwards added, that this appendage was figured in Mr. Carter's "Indian Journal, as far back as 1854"†. And yet, strangely enough, at page 44, he writes thus: "I am not quite certain that they (the villi) "are peculiar to *A. princeps*;" and he adds, "I have a drawing of an *Amœba* which has them, but does not appear to have the characteristic form of the nucleus of *A. princeps*. *If* they are confined to *A. princeps*, *then* they form a good distinguishing feature for this species; but, as I have before stated, they are not always present under the same form, and *sometimes not at all*" —a most important admission, as will presently appear.

As regards the so-called distinguishing character of *A. princeps* derived from its nucleus, I have only to remark that, when fresh and vigorous, the Hampstead specimens of *A. villosa* exhibited a spherical or slightly oblong nuclear cell-membrane— the nucleus itself being distinctly granular, spherical in outline

* It is worthy of note that, in the 'Annals' for 1856 (ser. 2. vol. xx. p. 33), out of all the various forms of *Amœba*, *A. princeps* is specially named as being closely allied to the sponge-cell, which is figured, and exhibits not a trace of the essential characters of *A. villosa*.

† Without cavilling at mere words, I cannot help thinking that the following expression of Mr. Carter's, coupled with what I am now stating, must engender an idea that he, and not I, pointed out the extrusion of effete matter from an orifice at the posterior portion of the animal :—"One point here is remarkable, viz. that while any part in front of the villous or posterior end may inclose a particle of food, it is only, so far as my observation extends (and in this I am confirmed by Dr. Wallich), the posterior extremity which gives passage to the egesta." (Annals, July, p. 35.) Mr. Carter then gives a reference to my statement to this effect in the previous number of the 'Annals'; but I have to repeat that, as he dwells so pointedly on his previous knowledge of an *Amœba* possessing the *villous* organ, and, after stating his intention of employing a nomenclature previously suggested by himself, employs mine without any acknowledgment, the inference I speak of, however unintentionally conveyed, is inevitable.

from all aspects, and occupying only so much of the vesicular chamber as to leave around it a clear hyaline space or ring, varying from about $\frac{1}{20}$th to $\frac{1}{10}$th of the total diameter of the organ, and that this ring was broadest, in proportion to the total diameter of the nuclear cell, in the smallest specimens.

Without stopping to discuss the propriety of placing in an entirely subsidiary light an organ of such importance as the villous appendage (even granting, for the sake of argument, that it had ever been previously included in any published definition of *A. princeps*), I own myself at a loss to understand how the character, specially alluded to as distinguishing that form from all other freshwater Rhizopods examined by Mr. Carter, could have been adduced under the circumstances; for, notwithstanding the "specific description" thus given of *A. princeps* at the commencement of his paper, at the close of that portion of it relating to *Amœba* we are distinctly informed that the villous appendage is "sometimes" altogether absent in *A. princeps,* and even the grand distinctive feature of the nucleus is inconstant; whilst, as if to add to the perplexity inseparable from the characters of *A. princeps,* as thus reconstructed, at the same time that, in Plate III. figs. 3 *d* & *f,* illustrating the paper on this form (Annals, July 1863), the absence of the pellucid ring around the nucleus is distinctly exhibited, in the same plate (figs. 2 *c* & *f*) the missing pellucid zone reappears in quite as marked a degree as in the figures of *A. radiosa* and *A. Gleichenii* appended to Mr. Carter's paper in the 'Annals' for 1856 (vol. xviii. pl. 5. figs. 4, 10, 17 & 18). So that the only characters left intact of those named in the introductory specific description are "the size of *A. princeps* when it is large, and the number of granules it contains" (Annals, July, p. 31). It need only be added on this head, that we are not left in doubt as to the size of the specimen depicted on the plate; for, instead of being under $\frac{1}{450}$th of an inch in length (see the next page), it is said in the explanatory references to be $\frac{1}{70}$th of an inch long, the nucleus itself (fig. 2 *c*) being $\frac{1}{700}$th of an inch in diameter, whilst in fig. 2 *f* it is as much as $\frac{1}{500}$th of an inch in diameter.

But I regret to say the difficulty of arriving at a clear view of the subject does not end here; for it seems doubtful whether the pellucid ring referred to as characteristic surrounds the nuclear cell-membrane, the nucleus within the membranous cell, or the nucleolus within the nucleus. This will appear on reference to the three subjoined passages:—

"It [the nucleus] is discoid in shape, of a faint yellow colour, and fixed on one side of a transparent capsule, which, being generally more or less large than the *nucleus* itself, causes the *latter* to appear as if surrounded by a narrow pellucid ring. In this state it is *invariably* present in *Amœba, Actinophrys, Spongilla*, &c." (Annals, ser. 2. (1856), vol. xviii. p. 221).

"While the distinguishing character of the nucleus [in *A. princeps*] to which I have above alluded, consists in the nucleolus being so much extended over the inner surface of the nuclear cell that it passes beyond the equatorial line of the latter, and thus causes the pellucid halo which is seen round the nucleus of other *Amœbæ* to be absent; that is, the nucleolus, being circular and of much less extent than the hemisphere of the nuclear *capsule*, in most *Amœbæ*, causes it to appear in them as if surrounded by a transparent area." (Annals for July, 1863, p. 31.)

"The nucleus in *A. princeps*, as before stated, differs in appearance from that of all other freshwater Rhizopods that I have examined in the absence of a pellucid area round the nucleolus." (Annals, July 1863, p. 39.)

Here we find the term *nucleus* at one time applied to the nuclear capsule, at another to the granular body lying within it; whilst, on the other hand, the whole of the granular body is at one time termed the nucleolus, at another the clear space within it receives that name. There can be no doubt that a vast deal of confusion has arisen here as elsewhere in referring to the *nucleolus* as if it were always a distinct portion of the nuclear structure, endowed with some distinct function. As referred to in my descriptions, the term nucleolus simply implies the central portion of the nuclear body, rendered more diaphanous than the marginal part by the partial or total absence of the granules, and the crowding of these bodies around the circumference.

Mr. Carter's views with reference to the feasibility of determining the appearances of so small an organ as the nucleus of *Amœba*, or tracing specific characters, where the specimen is small, are so diametrically opposed to all my experience that they demand careful examination. He says, "Whether" the nucleus, before the period at which the creature has attained the $\frac{1}{450}$th of an inch in length, "is circular and presents the usual pellucid area around it, or not, I do not pretend to determine, but I think it very likely; and then this state and the smallness of the *Amœba* would preclude all possibility of specific distinction; hence I do not think that there is any necessity for us to concern ourselves about the appearances. At this period the nucleus is not larger than a human blood-corpuscle, and the consistence of the nucleolus apparently homogeneous, that is, without granules, and composed of a fine delicate yellowish film of transparent plasma, in which state it continues, with the exception of increasing in bulk, up to the time when the *Amœba* has attained about one-tenth of the adult or maximum size, that is, about $\frac{1}{250}$th of an inch long."

Whatever difficulty Mr. Carter may have experienced in determining the minute characters of organisms not larger than a human blood-corpuscle, I repeat that I have, over and over again, observed the nucleus with its hyaline ring, the action of the contractile vesicle, granules, and the villous tuft in specimens of *A. villosa* not exceeding $\frac{1}{3330}$th of an inch in length, and $\frac{1}{10000}$th of an inch in breadth, the lens employed being a $\frac{1}{8}$th with an A eye-piece. I candidly confess, therefore, that it would have spared me no little pain had Mr. Carter abstained from making an observation so uncalled for, and at the same time so much at variance with his own experience.

In answer to Mr. Carter's statement respecting the impossibility of distinguishing the characters of an *Amœba* until it has attained the length of $\frac{1}{450}$th of an inch, it may suffice to mention that, out of the twenty-five so-called species of the genus, six are generally described as being under $\frac{1}{450}$th of an inch in length, even when full-grown; whilst a refutation of the view regarding the nucleus of such specimens will be found in some important observations (by Dr. W. Roberts of Manchester) on the minute structure of the human and other "blood-corpuscles," which appeared in the same Number of the 'Annals' (p. 60 of the present volume) as Mr. Carter's remarks on *Amœba princeps*.

But, irrespectively of the observations of others, those previously published by Mr. Carter himself contain abundant evidence that he has not always held the same opinion on this point; and it is only necessary to adduce one, out of several that might be brought forward, to show that the limits now assigned to the successful employment of the microscope have been considerably exceeded by him. Thus in his paper on the Infusoria of Bombay, published in the 'Annals' for 1856, we find a description and figures of *Euglypha alveolata*, exhibiting "ovules" with their capsules, both within and without the test—the nuclei of these clearly defined when $\frac{1}{4000}$th of an inch in diameter, an equally detailed representation being afforded of "the separation and development of granules into spermatozoids (?) *within the test*"—and, in one example (namely, *Astasia limpida*), a clearly defined view of the discoid ovules only $\frac{1}{6000}$th of an inch in diameter, but nevertheless "showing its capsuled character;" whilst in the 'Annals' for the succeeding year (vol. xx. p. 33), it is stated that certain bodies, $\frac{1}{5755}$th of an inch in diameter, "are polymorphic, and present the granule and contracting vesicles like the monociliated sponge-cell of the ampullaceous sac" [of *Spongilla*], and that "they also enclose particles of food." A figure is given of these bodies. Here, then, at all events, we have an Amœboid organism, with some of the very characters present which Mr. Carter has recently declared it to be impossible to trace with accuracy,—the *smallest* of my *Amœbæ*, as referred to, having been $\frac{1}{3330}$th of an inch in length, and these Amœbiform sponge-cells of Mr. Carter being $\frac{1}{5755}$th of an inch in diameter.

I may observe that few persons will be found to acquiesce in Mr. Carter's opinion (Annals, July, p. 39) as to there being no "necessity for us to concern ourselves about the appearance of the nucleus in *A. princeps* before it [the young *Amœba*] arrives at the size just mentioned," namely $\frac{1}{450}$th of an inch in length; for it will be admitted that we stand but a sorry chance of being able to trace out the development or reproductive process which results in the viviparous parturition recently described by me as occurring in *A. villosa,* and which forms a most important link in the chain of its vital phenomena, unless we do concern ourselves specially to investigate the configuration of the young animal even prior to its extrusion from its parent. The failure to do so will assuredly "preclude all possibility" of ascertaining the correctness or otherwise of those highly complicated reproductive phenomena which Mr. Carter has so zealously endeavoured to elucidate.

I would here mention having repeatedly observed, during the past month, young *Arcellæ,* varying in number from one to four, within the test of the parent. These young specimens were provided with a distinct test, the diameter of which was already so far in excess of that of the aperture of the parent test as to render their escape improbable otherwise than by its rupture. I have also seen what appeared to be full-grown *Arcellæ,* exhibiting every characteristic of the soft parts, but whose test was still soft and membranous, and surrounded the body somewhat loosely. Its surface, however, already presented traces of reticulation, but, instead of the usual inverted orifice, the margin of the aperture through which the pseudopodia protruded was corrugated externally, giving the structure the appearance of a medlar. Here, then, it would seem that viviparous parturition must necessarily be associated with the casting of the effete test of the parent and the development of a new one. In other respects, the occurrence of these young *Arcellæ* fully bears out Mr. Carter's detection of young individuals within the test of the closely allied *Euglyphæ* (Annals, 1856, ser. 2. vol. xviii.); whilst in both cases the phenomena may be regarded as analogous in their nature to the viviparous parturition described by me as observable in *Amœba villosa**.

* Perty records having "once seen two round motionless animals in *Arcella vulgaris,* each having a greater diameter than the mouth of the shell containing them." And he asks if these young *Arcellæ* are set free by the breaking up of the shell. Schultze also cites a similar example as occurring in *Gromia Dujardinii.* (See Pritchard's 'Infusoria,' London, 1861, p. 215.)

As also bearing directly on the characters of the Amœbina, I have to record an important fact which revealed itself during my examination of some of the material containing *A. villosa.* I allude to the detection in *Gromia oviformis* of a well-marked nucleus and nuclear vesicle. The contractile vesicle I failed to trace; but, in the presence of the manifest analogies between the *Gromidæ* and *Lagynidæ,* suggested by this discovery, it is extremely probable, I think, that this organ also may yet be detected. Should it be so, the transfer of *Gromia* from the lowest to the highest ordinal type of Rhizopod structure would be rendered necessary.

If the object now alluded to, in the paper on *A. princeps,* under the term "reproductive cells," be identical, as I suppose, with the "ovules" of Mr. Carter's former papers, these bodies must differ from the former in the very material point of not being nucleated. In the 'Annals' (ser. 2. vol. xviii. p. 223) the term "ovule is applied to a number of discoid or globular nucleated cells, which appear together in the sarcode of some of the Infusoria. At *an early stage,* in *Spongilla, Amœba,* &c., these bodies consist of a transparent capsule, lined with a faint yellow film of semitransparent matter, which, *subsequently becoming* more opake and yellowish, also becomes more marginated and distinct, and assumes a nucleolar form." That these bodies are the same seems certain, inasmuch as in Mr. Carter's recent paper they are spoken of as having been shown in *A. verrucosa,* under the first designation; whilst, on referring to the paper on that form and on *Spongilla* (Annals, vol. xx.), the general characters are identical with those of the bodies called "ovules" in the latter place.

But here, again, I am perfectly at a loss to reconcile the appearances and descriptions presented in one series of observations with those presented in the other. Above, it is stated that the ovules are "nucleated" (*loc. cit.*). In the 'Annals' for July 1863, p. 40, Mr. Carter affirms that he has "*on no occasion* been able to detect a nucleus in these cells, or anything like a germinal vesicle at *any* period of their existence—perhaps because it eluded" his "search." It is true he is now speaking of *Amœba princeps*; but, inasmuch as the *Amœba* with a villous appendage became known to him two years before he published his general characters of *Amœba,* in 1856, it is undeniable that marked and apparently exceptional characters must have been unnoticed by him. But, even as to the source whence these bodies primarily spring, it is impossible to arrive at a satisfactory conclusion regarding Mr. Carter's view; for, in opposition to the appearances presented by the ovules in their earliest state, as above cited from the paper of 1856, Mr. Carter now says, "At first they are delicate, and their capsules so undeveloped that they present the appearance of cells composed of nothing but a fine, delicate, semitransparent, homogeneous plasma; but as they grow older, this becomes granuliferous, and towards the adult state there is a distinct capsule," it being stated that they are the produce of the repeated binary division of the parent nucleus.

On the last-named head I cannot speak with certainty, but several reasons have led me provisionally to adopt a somewhat different view. Two of these may be mentioned more particularly. Mr. Carter says, "Of course, when present [viz. the ovules], there is no nucleus to be seen with them." (Annals, July 1863, p. 41.) Now, I can confidently assert that in specimens of *A. villosa*, charged with quite as large a number of these bodies as are described as having been counted by him in a specimen of *A. princeps*, the nucleus was present also; whilst in such as showed fewer sarcoblasts I constantly met with two, and now and then three, distinct nuclei, of almost equal size. And, again, in those individuals which contained sarcoblasts, the nuclei, whether single or multiple, were invariably less granular than those without them, the hyaline ring observable between the inner surface of the nuclear capsule and the nucleus itself of the latter specimens being more or less completely obliterated. But to this question I shall recur again presently.

From the description given of these bodies, now called "reproductive cells" by Mr. Carter, and which has appeared since my last paper was published, it is evident that I was in error when I stated my belief that the nucleated corpuscles of *Amœba villosa* (in contradistinction to the non-nucleated sarcoblasts) were probably identical with the "reproductive cells" of *A. princeps* (Carter). But inasmuch as I was ignorant, at the time my paper was written, that he had changed his view regarding the constitution of these bodies since the date of his previously published observations (1856 & 1857), it will be seen I had no alternative but to assume that, out of two kinds of corpuscles, differing from each other chiefly in the one being nucleated, the other devoid of nucleus, the kind presenting a nucleus corresponded with the "ovules" which up to that period stood described by him as possessing a similar feature.

I am glad to find, however, that the detection of this error, unavoidable as it was on my part, causes Mr. Carter's and my views regarding the reproductive office of his "reproductive cells" and my sarcoblasts to coincide in a great measure, although I am unable to confirm, by my own observation, the opinion entertained by him as to their being surrounded by a distinct membrane. But I cannot speak positively on the point until I have enjoyed further opportunities of studying the bodies in question.

On the mode of development of the nucleated corpuscles and sarcoblasts of *Amœba,* I have nothing to add to my previous observations; but I may avail myself of the opportunity to state that, in the earliest recognizable condition in which I have found Polycystina and Acanthometrina occurring as independent free-floating organisms at the surface of tropical seas, their rudimentary shell or framework* has invariably been enveloped in bodies precisely resembling the sarcoblasts of the mature forms. Since every gradation in size of these organisms has been met with by me, from that most minute condition in which they are scarcely larger than the large sarcoblasts found within the parent forms, to the fully-grown individuals,—and since the sarcoblasts of *Thalassicolla* have been met with by me in abundance, occurring both *within* and without the nuclear capsule, within and without the shell or spicular representatives of the shell in the form in which the latter are present, there can be no doubt, I think, that to this extent I have traced their share in the reproductive process. But whether any true reproductive act precedes their appearance or maturation, I have no evidence whatever to show; nor ought any evidence to be accepted as proof until the unbroken chain of attendant phenomena shall have been consecutively seen and described.

I may here mention that, as pointed out in *Amœba,* the nucleated corpuscles as well as the sarcoblasts have been detected by me in the Foraminifera, the Polycystina, the Acanthodesmidæ, Acanthometrina, Thalassicollidæ, and Dictyochidæ—all pelagic forms. In the Foraminifera the primordial segment is in reality the homologue of the omphalostype; and it seems by no means improbable that the coccospheres, already alluded to as constituting a phase in the development of some of the genera (as, for example, *Textularia*), may prove to be an advanced stage of their sarcoblasts. I have never seen a coccosphere within the chamber of a Foraminifer; but I may state that I possess numerous specimens of these bodies (from the single primordial chamber to the perfectly formed multiple segments of the shell) in which each chamber has retained the characters of the coccosphere to the last.

The first portion of the Amœban structure to which Mr. Carter draws attention he terms "the pellicula," stating that "*inference* leads us to the conclusion that there is a pellicle over the surface of *A. princeps,* however thin; and the fact that very frequently,

* As the earliest rudiment of the hard shell or framework of these organisms furnishes a most important character in their classification, I have applied to them the term *omphalostypes.*

on the application of iodine, the margin becomes of a deep violet colour, while all the other parts of the Rhizopod exhibit nothing but a more or less deep amber tint, seems to confirm it by chemical differentiation." Again, "Such a covering has been demonstrated by Auerbach in *A. bilimbosa,* and more satisfactorily, on account, probably, of the pellicula in this species being more rigid; but Auerbach does not show that it is coloured by iodine, although he figures starch-globules thus turned blue within it." .... "We must also infer that it is possessed of great elasticity and tenacity, so that it can yield a covering to the pseudopodia almost to any extent (as proved by the actinophorous rays of those Rhizopods which infest the cells of plants remaining after the sarcode has withdrawn itself into an interior or secondary cell); also that it admits of rupture (as in the introduction of food into the sarcode), and yet can heal over rapidly again. Thus it can undergo comparatively unlimited extension even to discontinuity, *but possesses no adhesiveness externally,* as evidenced by nothing adhering to it which is not seized and kept there by the instinct of the animal. Furthermore, in *A. princeps* the pellicula is allied to the cell-wall of plants by position, and, from chemical evidence (*i. e.* when treated with iodine), by an amylaceous composition." (Annals, July 1863, p. 32.)

In referring to the analogous organs of *Amœba* and *Serpicula verticillata,* Mr. Carter goes so far as to say, "The difference between cellulose and pellicula, and the absence of the vesicula, &c., are points which have so little [!] to do with the analogy in question when the latter is followed up through *Astasia, Euglena, Navicula, Closterium,* &c., into *Œdogonium* and *Nitella,* to *Serpicula,* that very little doubt will, I think, then remain of the offices of the nucleus in *Amœba* being similar to those of the nucleus of the plant-cell, whatever these may hereafter prove to be" (Annals, ser. 2. vol. xviii. p. 223), thus instituting a comparison between the plant-cell and a portion of the Amœban structure regarded by him as typical, but of which not a trace has ever yet been seen except in *A. bilimbosa* or the encysted state of other species, and then making this comparison a basis for assuming the identity in function of an organ which is present in the plant-cell as well as in *Amœba.*

It appears to me that an error of a serious nature is committed in associating the Rhizopods, whose bodies are polymorphous, with the Infusoria, whose bodies are monomorphous. Mr. Carter speaks of *Astasia* and *Euglena* as "freshwater Rhizopods" (Annals, ser. 2. vol. xviii. p. 227). But even here, I think, the distinction about to be drawn holds good, independently of differences in internal organization. In *Amœba villosa* we have a determinate indication, in a non-testaceous Rhizopod, of an anterior and posterior portion of the body, but nevertheless associated with a very high degree of true polymorphism. In *Astasia*, on the other hand, we have a definite shape of the body when at rest, but subject to variation when the creature is moving. *A. villosa* may be regarded, therefore, as a link, if need be. But the absence of a permanent aperture, either for inception or excretion of food, defines its position at the head of the Rhizopods; whilst the permanent "buccal tube" of *Astasia* marks that organism as belonging to a higher group.

With reference to the "diaphane" or ectosarc, Mr. Carter goes on to say, "This layer, as in other *Amœbæ*, lies immediately underneath the pellicula, and is distinguished from the sarcode or endosarc within by its greater degree of transparency and peculiar functions; for while the sarcode is clouded and presents a *rotatory motion*, the diaphane is clear and *distinctly endowed* with a locomotive and *prehensile* power. *Analogy* and actual observation would lead us to infer that, in certain if not in all instances, the ectosarc has the power of passing *through* (*sic*) the pellicula by rupture of the latter—a fact which becomes most evident when the pellicula is thick and resistant, as in *Amœba bilimbosa*, where it has been demonstrated by Auerbach, especially in his third figure of this species (Siebold und Kölliker's Zeitschr. vol. vii. p. 365, pl. 19. figs. 1–5, Dec. 1855)"*.

Before touching on the nature of the evidence on which the existence of the "pellicula" is based by Mr. Carter, I would direct attention to what appear to me to be contradictory characters assigned to that portion of the structure,—namely, elasticity so great as to enable it to yield a covering to the pseudopodia "almost to any extent," and yet such an amount of friability that "it admits of rupture (as in the introduction of food)"; for, since the pseudopodia are projected from the "diaphane" (ectosarc), and it is also the "diaphane" "which seizes the nutritious body, whether living or dead, animal or plant, surrounds it, and encloses it" (Annals, July 1863, p. 35), it is certainly difficult to conceive how the extreme elasticity insisted upon in the case of the pseudopodia should be completely cast aside in the case of the food-particles.

During my late survey of *A. villosa*, and after numerous carefully conducted examinations of the form usually known as *A. princeps* (from quite distinct localities, and kept separately from my specimens of *A. villosa*), I can only say I have never detected a trace of anything like a membranous outer investiture, except in the single individual referred to in the May Number of the 'Annals.' That specimen was in a state of nearly perfect quiescence, and apparently encysted; and consequently my experience

* Ann. Nat. Hist. July 1863, pp. 31, 32 & 33.

of these organisms leads me to the conclusion that nothing analogous to a pellicle exists, save during the period of encystation —for a similar reason that it would be unwarrantable to regard the capsule within which any of the other Protozoa are enclosed during their encysted condition as a true envelope belonging to the creature at all times. I may repeat that I have completely failed to render a membrane apparent even under the use of the customary chemical reagents. By employing iodine and sulphuric acid, I have coloured the external layer to some depth at times, and, as shown by Auerbach in his *A. bilimbosa,* have caused the granular and other contents to shrink towards the centre of the organism. But surely it demands much stronger evidence than is derivable from this experiment, to prove that the appearances so engendered are the exponents of a normal condition that previously existed, and not mere effects of chemical action on organic matter.

With due deference to M. Auerbach, I entertain the belief (based on appearances repeatedly seen by me in *A. villosa* when imperfectly defined under the microscope, coupled with those observed by me in the encysted specimen) that *A. bilimbosa* will prove to be either an encysted condition of another form, or one of the Protean phases of the typical form, namely *A. villosa.* The very striking character of the irregular portion of the surface shown in Auerbach's figure tends to confirm this view. This has been my opinion ever since the encysted specimen came under my notice; and I only hesitated to publish it in the hope of obtaining the encysted form of *A. villosa* in sufficient quantity, and with sufficient evidence of its being a transitional condition, to enable me to speak more confidently on the subject. Meanwhile I would simply direct attention to the fact, admitted by Mr. Carter, that the existence of the "pellicula" (except in those cases in which chemical reagents are employed) is wholly hypothetical,—and hence that the phenomena said to take place in it are equally so.

But my view with regard to *A. bilimbosa* is not an unsupported one; for, in order to put it to some degree of test, I have instituted the following experiments within the last two days.

Having killed some *Amœbæ* by holding a portion of the material containing them in a watch-glass over a spirit-lamp, I placed them under the microscope. The specimens were then motionless, and devoid of the usual contractile vesicle, but otherwise they scarcely differed in aspect from the living specimens. I now broke them up by carefully graduated pressure; and, by a slight displacement of the thin glass cover beneath which they were being examined, the detached masses were separated from each other. On dilute sulphuric acid and iodine being now applied, the result was similar to that produced in the case of living individuals,—with this exception, that the broken-up masses were not spherical, but irregular and ragged in their outline.

Here, then, the inference is legitimate, that, whilst vitality and contractility were destroyed by the heat so as to preclude the formation of ectosarc over the torn surfaces, the recession of the granular and other contents towards the centres of the masses yielded unmistakeable evidence that the action was purely chemical. But still nothing at all resembling membrane was evoked; and the tint imparted externally by the iodine was neither blue nor purple, but brownish; and, as in the case cited by Auerbach, some of the internally contained particles assumed a purple colour.

In *Amœba*, the true ectosarc appears to me to be nothing more than the outer layer of sarcode (for the time being) consolidated by contact with external influences, its depth (or, rather, thickness) being dependent on the length of time these influences continue to act upon it without intermission; whilst the consolidation referred to is greater at the immediate surface, and gradually diminishes in extent and finally fades away altogether from thence inwards. Leaving just now the question of reciprocal convertibility of ectosarc and endosarc, I would observe that this view is essentially similar to that propounded by Dujardin. It is corroborated, however, by a fact open to the observation of every one,—namely, that in the nearly quiescent condition of *Amœba*, when the outline becomes more or less spherical, the greater amount of consolidation of the exterior layer is shown by the hyaline margin becoming broader, and the whole of the contents being consequently made to recede towards the centre.

That an increased degree of consolidation does really exist in the outer layer of sarcode, and that the particles of which the entire body is composed are not held together only by the molecular cohesion of which we have examples in the formation of water-globules or oil-globules when placed in fluid media in which they are insoluble, I deduce from this fact, that whereas a foreign body, when of great size and resistent—as, for example, the large *Pinnulariæ* so frequently met with in the Hampstead *Amœba*, when fresh (Annals, April, pl. 8. fig. 4, and May, pl. 9. figs. 1–8, and in the *Actinophrys* figured in the number for June, pl. 10. fig. 4)—causes the outer layer to project almost to any extent without rupture (as in the last-named figure), the moment the body is torn asunder by pressure or other violence, such an object instantly slides completely out of the mass, and becomes liberated.

As to the power, spoken of by Mr. Carter, possessed by the ectosarc (*sic*) of passing through the pellicula, it will be seen, on reference to my paper in the 'Annals' for May (p. 370), that I distinctly point out this feature, and endeavour to prove by it, for reasons there assigned, that the ectosarc is gradually dissolved, as it were, when pierced by a newly projected mass of sarcode in the shape of a pseudopodium, in such a manner as to envelope a portion of the old ectosarc. Mr. Carter's figure (Annals for July, pl. 3. fig. 4) diagrammatically represents this condition, and, to my mind, clearly proves one of two things,—either that a new portion of ectosarc is instantaneously produced on the contact of the endosarc with the surrounding medium, or that, where such pseudopodia are projected, their characters must be of a different kind from the rest of the structure—an inference which is obviously not tenable for a moment. Lastly, I am unable to see that, by calling in to our aid any such process as *secretion* from the surface of the newly projected portion of sarcode, any more satisfactory explanation of the phenomenon is afforded; for it is obvious that, for every quota of ectosarc secreted, an equivalent quota would have to be re-absorbed, otherwise the whole body would rapidly be converted into ectosarc; whilst, assuming the process to be one of alternate secretion and absorption, the reciprocal convertibility of the ectosarc and endosarc for which I contend would be admitted *à priori* *. If not reciprocally convertible one into the other, as I have described, how is it that the contractile vesicle is sometimes single, sometimes multiple, in the same portion of the body—the multiple vesicles now performing their office separately and independently of each other, now coalescing with one another, so as to undergo their contraction in the shape of a single cavity? How is it that we constantly see a tentative double or multiple contractile vesicle—that is to say, two or more cavities separated from each other only by the most delicate films of protoplasmic substance which forms the partition-walls, these walls permitting the union of the contents on either side, not through a minute specialized aperture, but in a similar manner to the coalescence of two soap-bubbles? whilst on the next occurrence of distention at the same spot the contractile vesicle may appear in the shape of a single large cavity without supplementary ones. How is it that an *Amœba* may be lacerated so as to form two or more portions, each of which almost instantaneously presents, at every portion of the surface, the same appearances as existed prior to

* Mr. Carter speaks explicitly on this point. He says (Annals for July, p. 118), "That the diaphane, therefore, should pass into the pellicula, or the pellicula be secreted by the diaphane, seems untenable."

laceration, not necessarily by the folding together and union of the torn margins, but by the immediate development of ectosarc upon the torn surface? Let the process be called instantaneous cicatrization, or what else we will, the phenomenon remains the same.

Again, let me ask what prevents the food-vacuole * from collapsing suddenly when relieved of its contents by absorptive digestion, as often happens? Admitting that the watery contents prevent collapse from taking place, why do not all the vacuoles, when crowded together, as they frequently are, coalesce, instead of remaining for the most part distinct from one another? † And lastly, why do the globules of sarcode, when extruded under pressure by rupture of some part of the surface, and floating side by side (as described and figured by me in the 'Annals' for May, p. 370, pl. 9. fig. 8), show no tendency to coalesce, unless it be that the inner layer in the former case, and the outer layer in the latter, by which each globule is surrounded, instantaneously becomes converted into ectosarc by simple contact with the surrounding medium?

Chemical reagents, when applied to a mass of sarcode, prove nothing beyond their effects on that substance; that is to say, they do not demonstrate the primary presence of a membranous layer, even where they succeed in producing the semblance of one. And this is the case without reference to the well-established fact that certain chemical substances frequently render more distinct structures which are already imperfectly visible or demonstrable without their employment.

It is obvious that when a food-particle is incepted by an *Amœba*, the vacuolar cavity receiving it must either be formed of ectosarc or endosarc, or of both combined. If it be urged that it is composed of the former, it follows that, at every inception of food, so much ectosarc as is requisite to surround the object must be abstracted from the general surface of the body. Hence, when the quantity of ingesta is large, as frequently happens, the greater part, if not the whole, of the ectosarc must speedily be conveyed into the interior, leaving the viscid

* See my paper in the 'Annals' for June, p. 436.

† It will be recollected that I have endeavoured to prove, by the mode in which foreign bodies are incepted as food, that the food-vacuole is formed either of an intussuscepted portion of the ectosarc around the point of inception, or, supposing the food-particle to be forced through the ectosarc by the rupture of the latter, that the simultaneous admission of a portion of water at once converts the endosarc, of which the boundary of the cavity is formed, into ectosarc. It is by this means that the entire food-vacuole is sometimes extruded through an orifice in the villous region—a thing which could not take place were the food-vacuole formed of *endosarc*.

surface unprotected. Such a view is therefore untenable; and, as I have endeavoured to show, the appearances are only reconcilable with one or other of the following processes: that is to say, the food-particle, on being dragged to the surface, or surrounded, as the case may be, either penetrates the ectosarc and finds its way into a cavity extemporized in the endosarc, or, the cavity being formed partly by the inversion of a portion of the ectosarc, which is thrust in, as it were, before it, the sealing up of the food-vacuole is effected by a portion of endosarc. In the first case, the mere contact of the endosarc with the portion of water which is admitted along with the incepted object converts it into ectosarc. In the second, that part of the food-vacuole which does not already consist of ectosarc is converted into it by the same means. But under no circumstances have the appearances been such as to lead me to the inference that the food object passed into the interior in the same manner that a stone does when slowly dropped into water.

It might, at first sight, be imagined that the food-vacuole is a simple cavity produced within sarcode by the presence of a foreign body, after the fashion of a globule of oil in water, since the incepted masses at times present no appreciable vacuolar space around them; or that the endogenous vacuolation, to which reference has been made, negatives the above view. To the first of these objections I would answer that the vacuole, when present, undoubtedly contains watery fluid; and it appears almost certain that a distinct coagulative effect is produced in the endosarc by contact with it, from what takes place when effete matter is extruded through a tubule in the neighbourhood of the villous organ. We then perceive that, on reaching this region, the contractile power is so great as to cause the vacuole and its contents to move towards the margin, and the egress of the effete matter proceeds slowly till its largest diameter has passed outwards. When this has happened, the effete object slips out with a jerk, whilst the residue of the contractile effort causes the vacuolar spherule to assume a tubular and, very frequently, an infundibuliform shape, similar to that described and figured by me in the 'Annals' for May. But at this point the special contractile effort ceases, and hence the consolidated layer constituting the wall of the tubule requires a considerable period for its reconversion into endosarc, which proceeds from within outwards. In this case, it is very evident, I think, that, did no difference exist between the degree of consolidation of the tubular wall and the endosarc by which it was immediately surrounded, the reconversion spoken of, and the consequent obliteration of the excretory tubule and its external orifice, would be comparatively instantaneous, instead of occupying, as it generally does, a period varying with its size, from a few minutes to upwards of an hour.

Here, then, we have the strongest evidence that the degree of consolidation necessary to establish the differentiation of the ectosarc, so as to permit a tubule or excretory passage to be formed, the walls of which do not instantly coalesce as water does around any heavier object dropped into it, but close slowly and gradually from within outwards, is due to the mere contact of the fluid which is invariably present whenever such tubules or excretory orifices are observable; whereas, in those cases in which the watery matter has been removed by digestive absorption prior to the discharge of an effete mass, the latter passes out through the substance of the ectosarc, and without the production of any passage whatever. In this case, moreover, the ectosarc closes around the effete body almost as rapidly as that body can escape.

When foreign substances appear within the endosarc, unsurrounded by any appreciable vacuole, I have almost invariably found them to consist either of mineral particles or the effete remains of food objects. But this by no means proves that they obtained entrance into the interior without any accompanying water, but only that the latter has been absorbed; for, in view of the conditions and the manner in which a foreign body is invariably engulfed, it seems almost impossible to conceive the entrance to take place without the simultaneous entrance of a portion of the medium in which both the animal and the food-particle are sustained.

The reciprocal convertibility of endosarc and ectosarc, for which I would propose the term *amœbasis**, constitutes, as it appears to me, a very important and definite distinction between the animal and the vegetable protoplasm,—the permanent differentiation of the true cell-wall of the protophyte rendering necessary nutrition by endosmotic absorption, whereas in the Protozoan the continual interchange of parts enables the animal to incept organic matter for food. But, as I shall endeavour to show on a future occasion, this power of incepting solid organized substances does not present itself distinctly in the two lower orders into which I propose to divide the Rhizopods, but only in the third or highest order, in which the contractile vesicle makes its appearance for the first time; whilst, as already mentioned in my paper in the 'Annals' for June, p. 440, if a

* Ἀμοιβή (reciprocity). It is somewhat singular that the word from which the generic name of *Amœba* is derived, and which was selected with reference to the alternate expansion and retraction of the pseudopodia, should in reality express the precise action now referred to as being involved in the sarcode substance.

boundary line exists between the Rhizopods and the true Infusoria, it consists in there being, amongst the former, no permanent orifice either for the inception or extrusion of foreign or effete matter, and the phenomena of amœbasis are present; in the latter, whatever parts exist are permanent formations, and there is either a single or dual orifice for the inception and extrusion of substances used for food.

"Of the peculiar and particular function of the *sarcode*," says Mr. Carter (Annals, July 1863, p. 36), "*there can be no doubt*, viz. that of digestion." Now, without calling in question the function, I may be permitted to observe that Mr. Carter takes for granted a most important histological as well as physiological distinction between the ectosarc and endosarc, which has only been entertained by Cohn with regard to the Infusoria, as far as I am aware,—namely, that the ectosarc ("diaphane") is formed from the "sarcode" (or endosarc), and that, "since it has a distinct structure as well as office, having been produced, it is not reconvertible into any other organ by any process but that of digestive assimilation" (Annals, July 1863, p. 37).

So that having, in the first place, assumed a histological distinction between endosarc and ectosarc, the existence of a special function is likewise assumed in one, and its absence in the other, whilst an analogy is insisted on between a lower and a higher grade of organisms,—solely, as it would appear, on the ground that the microscope has failed, in both cases, to render visible specialities of structure for the existence of which there is not a vestige of evidence!

I am also compelled to avow that the theory put forward by Mr. Carter regarding the pellicula "possessing no adhesiveness, as evidenced by nothing adhering to it which is not seized and kept there by the instinct of the animal," is not reconcilable with a fact to which I drew attention in the 'Annals' for April (p. 288), or with his subsequent admission to the same effect, contained in his paper (July, p. 43), notwithstanding his previously expressed opinion (p. 32), namely, that the villi exercise a distinct prehensile faculty, and one which unquestionably resides in the *external* layer of which they are composed, and is quite independent of any grasping action, such as we witness in the rays of *Actinophrys*.

Before quitting the subject now under discussion, I may mention that a vast fund of light has recently been thrown on "the development of the organic cell" by Professor H. Karsten, in a paper to which I shall have occasion to refer more in detail at some future opportunity. At present I would merely state that we are indebted to him for having been the first to advance a definition of "cell"-structure conformable with the organization of the Rhizopods, at the same time that it proves they cannot be regarded as unicellular. For although my experience of Rhizopod structure compels me to deny the normal presence of such an investiture as might legitimately be termed either membranous, capsular, or vesicular—whatever may be the true state of the case as regards *A. bilimbosa,* or the encysted condition of any other form, I regard the exterior of *Amœba* as falling strictly within the definition of a cell-"*wall,*" as propounded by Professor Karsten, the outer layer or ectosarc for the time being, however indefinite, constituting the homologue of the cell-membrane of the higher Protozoa and Protophyta; whilst the facts connected with the truly cellular nature of the sometimes single, sometimes multiple nucleus demonstrate the truth of the concluding sentence of that author's paper. His views are summarily embodied as follows:—

"The primitive form which matter capable of organization assumes is that of the vesicle—the cell, inseparably composed of membrane (wall) and contents. Each of these two constituents of the elementary organ, constantly exerting the most intimate influences upon each other, is capable of advancing further in its development by the aid of the *physico-chemical forces to which it is indebted for its existence.*" And again, "Owing to the complicated structure of the tissue-cells which enter into the composition of developed organisms, it is erroneous to speak of unicellular plants and animals. With as little reason can we imagine cells without membranes; such bodies, in my opinion, should be designated drops or granules" *—thereby confirming the opinion I guardedly expressed when speaking of the true significance of a membranous nuclear cell in *Amœba villosa* (Annals, June, p. 438).

The basal sarcode in *Amœba,* and probably in all the lower animals, is generally regarded as a homogeneous, colourless or nearly colourless, hyaline mucus, within which a number of extremely minute granules are suspended. This granularity, coupled with a high refractive power, serves at once to distinguish sarcode from water, and hence enables me to affirm that the clear space surrounding the nuclear mass of *A. villosa* is composed of this substance.

It will be observed that there is a discrepancy between Mr. Carter's and my estimate of the size of the crystalloids, his measurement of the largest met with being recorded as $\frac{1}{2000}$th of an inch in length, whereas my largest is only $\frac{1}{4500}$th of an inch in length. But inasmuch as Mr. Carter states that the

* Translated by Dr. Arlidge from a separate impression from Poggendorff's 'Annalen' (vol. cxviii., Berlin, 1863), and published in the 'Annals' for July 1863.

specimen he alludes to "was composed of an irregular crystalline *aggregate*, based *apparently* upon an octahedral form," the two measurements of the single crystalloid are probably nearly identical.

Contrary to the opinion expressed by Mr. Carter, I found that the crystalloids of *A. villosa* are of the hexahedral series*, and occur as such even in the smaller specimens. Whether the crystalline state be the primary one or not, I am at present unable positively to say, although it seems highly probable; for the association with them of rounded granules, of nearly similar size, in some but by no means in all specimens, although perhaps indicative of the latter being a rudimentary condition of the former, cannot be accepted as a proof of the fact, any more than that in the oldest specimens, which sometimes present both the granules and the crystalloids, the former necessarily constitute a disintegrated stage of the latter. In my Streatham specimens of *A. villosa,* when first procured, the roundish granules were almost entirely absent. Now (July 3) they are nearly as plentiful as the crystalloids. On this head I have only to express my obligation to Mr. Carter for calling to my recollection that I had inadvertently omitted to allude to Auerbach's discovery of crystalloids in *A. bilimbosa,* although fully alive to the fact when I penned my paper—more particularly as Auerbach regards the crystalloids as hexahedral, which is the view I adopt with regard to those of *A. villosa* and the other forms in which those bodies have been detected by me (Annals, June, pl. 10. fig. 7).

Mr. Carter says that he observed the villous appendage in 1854; but it would appear that he failed to recognize its nature or office; for, writing in the 'Annals' in 1856 (vol. xviii. p. 116), the following passage occurs:—"Finally, when all activity ceases and the *Amœba* becomes stationary (by fixing itself to some neighbouring object through a pedicular prolongation of the *pellicula*†), a new layer of the latter is formed below the old one, and thus a capsule is formed, and the pellicula replaced on the body of the *Amœba,* until the latter becomes firmly encysted. To what part of the body of the *Amœba* the pedicular process corresponds I am ignorant; but it is interesting to see that in *Euglena,* where a similar process takes place, it is the *anterior* extremity which is next the pedicle." This is precisely the *reverse* of the position of the prehensile portion in *A. villosa,* unless, indeed, Mr. Carter means to convey that the villous region

* I have succeeded in mounting these crystalloids in balsam, by which their true shape is very distinctly brought out under a $\frac{1}{8}$th or $\frac{1}{12}$th objective.

† Proving that at this period he entertained a different view with regard to its adhesive quality.

is not invested, as he supposes the rest of the body to be, with the "pellicula," which I imagine is not the case, from what he says in the 'Annals' for July 1863. In that paper, at page 31, he says the "pseudopodia proceed from a *posterior* end which is normally capped with a tuft of villous prolongations." It will be seen that this expression admits of two diametrically opposite interpretations; that is to say, it may either mean that the pseudopodia are projected *from* (in the sense of the opposite direction to) the villous appendage, or that they are actually projected from the midst of the villi themselves. If we accept the first interpretation, it is evident that Mr. Carter, when describing the characters of *Amœba* generally, in 1856, must have been unaware of the true significance of the villous appendage; for he referred to *Amœba Gleichenii*, and not *A. princeps*, in order to exemplify the prehensile organ of the genus. In doing so, moreover, he says, "To what portion of the body of the *Amœba* the pedicular *process* corresponds I am ignorant. But it is interesting to see that, in *Euglena, where a similar process* takes place, it is the *anterior* extremity which is next the pedicle"—that is to say, the opposite extremity to that in which it occurs in *A. villosa* or *A. princeps*.

On the other hand, if we accept the second interpretation, as already pointed out, it is altogether irreconcilable with the appearances presented, which may be seen at a glance on examination of *every* form exhibiting the villous appendage.

I have seen no reason to call in question the generally received opinion that, after each contraction, the contractile vesicle reappears at the point of obliteration, or in immediate contact with that point. Alluding to this fact, Mr. Carter (in the 'Annals' for 1856, vol. xviii. p. 128) says, "We may perhaps *infer* that the situation of the vesicula in *Amœba* and *Actinophrys also* is fixed, though, from their incessant polymorphism, it appears to be continually varying in position." In the case of *Amœba villosa*, however, the polymorphism does not interfere with observation; and hence it becomes manifest, at a glance, that the contractile vesicle reappears as above stated—the villous organ, in the midst, or at the margin, of which the contraction invariably takes place, affording a fixed point for comparison. In *Actinophrys Eichhornii*, again, when examined on a slide under a thin glass cover, there is no difficulty in obtaining a tangential position of the contractile vesicle at the same time that the body of the creature is kept immoveable; and we thus obtain a perfect view of the alternating action. But in the latter species I have never detected anything like supplementary vesicles given off from the primary one, or any appearance indicating that the contractile vesicle is in direct communication with the vacuolated sarcode around.

Owing, doubtless, to an unintentional alteration of my description of the contractile vesicle of *A. villosa,* Mr. Carter makes it appear, however, that I assume the possibility of its formation as in the case of spontaneously formed vacuoles, at any portion of the body. Thus (Annals, July 1863, p. 39) it is stated that I regard "all these dilatations as extemporized vacuoles;" whereas I draw a marked distinction (Annals, June 1863, p. 439) between the contractile vesicle, to which I refer as "a *specialized* vacuolar cavity," the "food-vacuole, which is invariably formed at the surface," and those endogenous vacuoles which appear and disappear spontaneously within the substance of the organism (*loc. cit.* p. 436). The grounds for these distinctions will become manifest as I proceed. Meanwhile I would direct particular attention to the definition of the contractile vesicle given by Dr. Carpenter ('Introduction to the Study of the Foraminifera,' p. 14), namely, "*a vacuole with a definite wall,*" inasmuch as I shall hereafter endeavour to prove that to this extent only can it, with propriety, be regarded as a distinct structure.

In allusion to my remarks in the 'Annals' for June (p. 439), Mr. Carter says he is glad to find that I support him in the opinion that the contractile vesicle of *Amœba* discharges itself externally. As stated in the 'Annals' for June (p. 441), it will be seen that I had also satisfied myself of the fact with regard to an Infusorial animalcule. It is right, however, to mention that, so long ago as 1849, Dr. O. Schmidt asserted that the contractile vesicle in *Actinophrys* opens externally—although Dr. Lachmann, from whose writings I obtain this piece of information, is of a contrary opinion*. But Mr. Carter inadvertently omits to state that the determinate portion of the body in *Amœba* at which the discharge of the contractile vesicle takes place was pointed out, for the first time, as observed in *A. villosa*; for when I quoted his very graphic description of the action of this organ as occurring in *Amœba* and *Actinophrys* (Annals, 1856, vol. xviii. p. 126), I was certainly under the impression, from what was advanced in the same place (see next paragraph), that, in indicating a definite spot at which the discharge takes place in *Amœba,* my opinion was at direct variance with his. Thus, although Mr. Carter, in his recent paper (Annals, July 1863, pp. 38 & 39), says, "It is a remarkable fact, that although the vesicula is borne round the interior of *A. princeps*

* Dr. C. F. J. Lachmann on the Organization of the Infusoria (Ann. Nat. Hist. 1857, ser. 2. vol. xix. p. 227).

with the sarcode to which it belongs, it only discharges itself in the neighbourhood of the villous or posterior end; and such is the case also with the egesta of the digestive spaces; so that one might also *infer* that there was a particular aperture through the diaphane and *pellicula* at this part of the *Amœba* for this special purpose, as we see in most of the other Protozoa, where the vesicula is stationary, and frequently fixed close to the anal aperture,"—in his observations on the contractile vesicle, published in 1857 (Annals, ser. 2. vol. xvii. pp. 356 & 357), he writes as follows:—"All the internal organs are imbedded in it [the sarcode], part of which are fixed, and part moveable; it is also the receptacle for food, which, in the *Amœba,* passes *into and out of it,* directly through the diaphane, *as they have no special apertures of external communication* for this purpose;" and, as already stated, the latter view remained unaltered in any of his published papers, up to the date of his recent notice on *A. princeps*.

Having thus far shown the grounds on which Mr. Carter now infers the existence of a permanent excretory aperture through the diaphane and pellicula, which, according to the above admission, invest the sarcode-substance at the villous region, I would adduce the evidence upon which I have arrived at an opposite conclusion, and accordingly consider the excretory orifice as being neither a permanent portion of the structure of the contractile vesicle nor of the ectosarc of the villous organ.

Premising that the following details have chiefly been gathered from *Amœba villosa* and its protean varieties, I have to observe that, in its collapsed quiescent state, the contractile vesicle presents the appearance of a minute villous tuft suspended freely within the endosarc. When the specimen is tolerably free from foreign objects, the structure of the contractile vesicle can readily be made out whilst it remains quiescent near the villous organ, and then the identity in the intimate structure of the two parts becomes at once manifest. This is a material point, since it lends strong confirmation to the view, that whatever the mode in which the excretory orifice is produced in the one organ, it is in like manner produced in the other. But to this subject I shall more fully revert hereafter.

During the complete contraction of the contractile vesicle no internal space is discernible. This is probably owing to the consolidation of the ectosarc of which the minute villi are composed engendering a slight degree of opacity. The external surface of the contractile vesicle, however, can readily be distinguished as being composed of a number of minute papilliform villi, closely appressed, and imparting so rough an outline to the organ that it is somewhat difficult to believe that it can be identical with the hyaline and brilliant globule presented to view when observed in its state of greatest distention. The transition, however, is gradual, and leaves no room for doubt on this head. Sometimes the diastole* is altogether confined to the main cavity of the organ. When this happens, the central diaphanous space which shortly presents itself increases slowly in dimensions, whilst *pari passu* the boundary-wall becomes thinner, the villi grow shorter†, and the opacity is exchanged for an almost crystalline transparence. In this condition, the remains of the little villi can be faintly detected, under a sufficient magnifying power, as minute spots, distributed sparsely and unequally over the surface of the vesicle. But no trace of a double outline is visible, even under the highest power of the microscope; nor does its boundary wall approach more closely to the appearance of distinct membrane than the boundary wall of an oil-globule. Indeed, but for the scattered papillæ on its periphery, it would be absolutely hyaline throughout, and barely distinguishable from a solid globule of sarcode. In this, its fully distended state, no supplementary vesicles are evolved from any portion of its surface. During the systole, the appearances are reversed in their order, and take place in a much shorter period—the hyaline clearness becoming first destroyed, and the faint spots growing, as it were, into a crowd of villi, until finally the whole mass resumes its pristine aspect. But now and then the systole seems to be checked before completion, and the diastole recommences without entire obliteration of the cavity. Again, instead of the diastole originating at a single point, sometimes from two to twenty minute globules start into existence around or near that point, and cover a space considerably in excess of that occupied by the collapsed primary contractile vesicle. These globules are cavities formed within the villi, which thus become temporarily converted into cæca, admitting of distention to a certain point, and then either bursting into each other or into the primary cavity, as the case may be; whilst at other times one or two, but very rarely more, of the supplementary vesicles thus formed become altogether detached, after the fashion of a soap-bubble given off from a pipe‡, and circulate amongst the rest of the particles within the endosarc of the *Amœba*. When this occurs, I have

* Although the organ in question bears no analogy to the heart of the higher animals, as it contracts and expands rhythmically, the terms diastole and systole may be employed without impropriety, in order to distinguish the action more clearly from that of the ordinary vacuoles.

† An analogous effect is produced when a caoutchouc capsule, the wall of which is tuberculated and opaque in its unexpanded state, is inflated until the entire surface assumes a homogeneous and semidiaphanous appearance.

‡ Mr. Carter suggests this simile in describing the disengagement of

now and then distinctly seen the tubular isthmus which connects the supplementary with the primary cavity contract, become by degrees attenuated to a mere filament, and finally part in the middle, its conical-shaped ends gradually melting into the boundary-wall of the primary contractile vesicle on the one hand, and the supplementary vesicle on the other.

Both seem now to be wholly independent of each other*. The primary vesicle may either go on performing its diastole and systole without moving from the villous margin, or may take part in the pseudocyclosis. The supplementary one, again, may move away to the opposite or anterior extremity of the *Amœba*, changing its relative position to the villous organ and the primary contractile vesicle in every possible manner, and apparently for an indefinite period, and may ultimately return to discharge its contents independently at some portion of the villous region distinct from that occupied by the primary vesicle, or may actually find its way to the parent from which it sprang, and coalesce with it, reappearing, or otherwise, on the next diastole of the primary organ, as the case may be. These supplementary contractile vesicles rarely present papillæ on their surfaces; when they do so, these are very few in number; so that it is almost impossible to determine whether the object we are looking at be an ordinary empty vacuole or a contractile vesicle, unless we continue our observations over a period sufficiently protracted to embrace the next systolic action.

Lastly, it is deserving of special notice, that whenever the identification of one or more supplementary contractile vesicles

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"digestive spaces" at the inner extremity of the "buccal tube" of *Paramecium*, &c. (Annals, 2nd ser. vol. xvii. p. 357).

* In Dr. Carpenter's 'Introduction to the Study of the Foraminifera,' it is stated, on the authority of MM. Claparède and Lachmann, that in a species of *Amœba* allied to *A. princeps*, after the contraction of the contractile vesicle, from four to eight vacuoles were seen to spring up at different parts of the body, often at a considerable distance from the contractile vesicle, and that these seemed to move towards the latter when they had attained a certain size, and discharge their contents into it. In a note, Dr. Carpenter states his belief that distensible vacuoles have been mistaken, by some observers, for multiple contractile vesicles, but that they have not the well-defined boundary of that organ, and they do not present the rhythmical contractions.

According to my experience, no vesicle, unless it be a true contractile vesicle, under any circumstances bursts into the primary one.

According to my experience, contractile vesicles or supplementary contractile vesicles, when detached, may burst into each other, but never into vacuoles, or *vice versâ*; I cannot help thinking, therefore, that the "vacuoles" which are here spoken of as seeming to burst into the contractile vesicle must have been supplementary vesicles, not evolved spontaneously in the substance of the endosarc, but disengaged and moved to a distance from the primary one before the observation commenced.

has been rendered possible, owing to their having been continuously watched from the moment of their evolution, neither they nor the primary contractile vesicle from which they were evolved coalesce with the *vacuoles* or with the nuclear capsule, even when powerfully appressed against each other. They coalesce, however, with each other when they happen to come in juxtaposition during their movements to and fro, even at a distance from the villous region. But they neither perform their systole singly nor when so coalesced, until they once more reach the posterior or villous margin.

Now, assuming, for the sake of argument, that the primary contractile vesicle is furnished with a fixed and determinate orifice for the discharge of its contents, and that a corresponding orifice occurs at some spot on the villous surface, it is quite obvious that the coincidence of the two apertures can only be maintained, in an organism of so polymorphous a nature, as long as the contractile vesicle and the villous appendage maintain an undeviating relation to each other. But it has been shown that this is not the case in *Amœba villosa*; for the location of the vesicle at the spot where it discharges is only temporary, and its movements, when detached from that spot, conclusively prove that all union whatever between the wall of the vesicle and the villous region, apart from that provided by the general protoplasmic substance constituting the interior of the body, is destroyed. Besides this, I am inclined to believe, from the appearances (although I cannot speak positively to it as a fact), that the discharging orifice is not always in the same spot of the villous surface, but that its position, although restricted to that portion of the animal's body, varies with the polymorphic character of the villous organ itself and the situation it assumes relatively to the nucleus or other contents when resting in the vicinity. In the case of the supplementary vesicles formed each in one of the minute villous cæca, the isolation from the primary vesicle and from the villous appendage is quite as certain; for, owing to their being generally of smaller diameter, these supplementary vesicles move about with greater freedom, passing in every direction round or along the different aspects of the primary vesicle when at rest or when it also happens to be roaming about the centre of the body, and, for the time being, constituting as distinct organs as if they had been derived from separate sources. This being the case, it seems, as already urged, almost impossible to conceive that any permanent bonds of union, such as sinuses, or any determinate apertures, should exist either in the primary or supplementary vesicles. With regard to the non-existence of a determinate and constant excretory orifice at the villous surface, the evidence is quite as conclusive.

In the first place, no such permanent orifice can be detected even with the aid of the highest powers of the microscope and every essential accessory in manipulation. I am aware that this may be regarded as inconclusive by some persons; but, whilst I am quite as ready as Mr. Carter to believe that our optical appliances frequently fail, even under the most favourable circumstances, to resolve extreme subtleties of organic structure, I conceive that, in the example under notice, this evidence is not of the purely negative character that it would be were no trace discernible of the process whereby the contents of the contractile organ, or the effete matter within the food-vacuoles, are extruded at the surface.

The excretory aperture is extemporized, and its closure takes place from within outwards, solely because the indefinite consolidation of the sarcode, to which the name of ectosarc has been very appropriately given by Dr. T. Strethill Wright, being at its maximum at the immediate surface in contact with the medium around, and decreasing in degree from the surface inwards, the same cause that prevents the coalescence of the pseudopodia of *Amœba* under ordinary circumstances, in the first place increases the resistance to the passage of the object about to be extruded as the surface is approached, and, in the second, causes the coalescence to take place from within outwards, and its rate to depend upon the degree of consolidation attained by the ectosarc (see p. 132). Hence (and this is a most important fact), whilst the viscidity of the endosarc, when an *Amœba* is suddenly torn across, enables foreign bodies to slip out as they do from a globule of oil (that is to say, without driving a layer of the substance before them as they escape, or leaving a depression on the surface behind them), the comparative rigidity of the ectosarc causes a generally infundibuliform tubule* or pit to be formed, which tubule or pit coalesces from its inner pointed extremity in the direction of the exterior, and, finally, becomes altogether obliterated. In the least active condition of *Amœba villosa,* when several villi frequently combine to form single larger ones, the latter are often so hyaline as to render the detection of anything like a canal inevitable, did it exist; and it is in these that the mode of formation of the excretory tubule and its closure can be so clearly made out as to leave no doubt on the subject; for as the point of the tubule slowly advances outwards it leaves behind it a perfectly hyaline tract, the appearance presented during the process of closure being precisely similar to that observable in a thermometer-stem

* See my observation on the infundibuliform tubule in the 'Annals' for May 1863, pp. 366, 367.

where the capillary channel has been somewhat extended and sealed up at one end under the action of the blowpipe.

In a former communication ('Annals,' May, p. 367) attention was drawn by me to the occasional occurrence of a funnel-shaped tubule which opened out in the midst of the villous organ; and it was stated that when this took place, no contractile vesicle was observable. It was also stated that I had seen effete particles, and, on three occasions, bodies which resembled vacuoles, extruded through similar orifices. More recent observations, however, have satisfied me that the failure to detect the contractile vesicle during periods which I then considered sufficient to ensure the occurrence of the diastole or systole may have been due to the insufficiency of those periods, and hence that this organ may have been present notwithstanding its having escaped notice. The guarded manner in which I stated what took place was the result of a doubt as to whether the tubule was formed by the contractile vesicle, or by a vacuole, or was in reality an extemporized channel. The opinion I now hold—one based on actual observation—is, that whereas in some cases a food-vacuole may be reabsorbed into the substance of the body after the effete matter it contained has escaped, and in this way be converted into an infundibuliform tubule, in others the vacuole may be discharged along with the effete matter which it encloses, and the tubule may be produced in the substance of the body at the point of extrusion,—the first of these appearances presenting itself when the effete mass is of a shape admitting of easy discharge as soon as the margin is reached, the second when the mass is so irregular in outline as to entangle its own vacuole and carry it along with it.

According to my experience of *A. villosa*, it seems almost certain that, normally, the contractile vesicle is single, and that the evolution of supplementary vesicles from the primary one, in the manner already described, may take place without reference to approaching fission. For, did the evolution invariably precede that process, we should, in all probability, detect a plurality of nuclei also, which is not always the case. And, unless we regard fission in these lower organisms as an accidental phenomenon, the supplementary vesicles, when once detached in such cases, would not coalesce again with the primary one. On the other hand, there is every reason to believe that, when fission takes place normally, each segment is provided with its own nucleus and contractile vesicle. I say normally, because examples have been observed by me, from the commencement to the end of the process, in which sometimes the nucleus, and sometimes the contractile vesicle was absent in one of the newly formed segments. But I must mention that, whenever the former has been absent, the segment remained comparatively torpid and motionless, whilst the segment provided with this organ moved away energetically as soon as the separation was complete*. Under these circumstances it has yet to be determined whether the contractile vesicle at any time originates spontaneously, or is invariably an integral part of the organism. Judging from its presence in full activity in the minute viviparously produced *Amœbæ*, the latter conclusion seems to me most probable. But, I need hardly say, the point is one that demands a great deal of careful investigation before it can be regarded as settled.

Mr. Carter ('Annals,' 2nd series, vol. xviii. p. 129) observes, in allusion to the occasional plurality of the contractile vesicles in *Chilodon cucullulus* and the Rhizopoda generally (*loc. cit.* p. 130), that "the synuses of this system the sarcode of *Amœba* not only seem to burst into each other and into the vesicula, but, when the latter has contracted, another sinus, partially dilated and situated near the border, may be seen to swell out and contract after the same fashion before the reappearance of the vesicula,"—a figure (plate 7. fig. 81 *a a*) being appended in which two contractile vesicles, in a partially distended state, are represented on *opposite margins* of the body of *A. quadrilineata*, and described in the explanatory text (p. 248) as being "about to discharge themselves independently of the large, apparently normal one," which is centrally placed between them at a considerable distance from the true posterior extremity of the body.

In describing the contractile vesicle of *A. villosa* in the 'Annals' for April last (p. 289), I mentioned that it sometimes presented a reticulated appearance. I have repeatedly seen the same appearances since then, and have no doubt now that each contractile vesicle is able to project from its wall supplementary vesicles at points answering to the reticulations or, as I now regard them, villi. But, whilst it is quite possible to conceive that the contractility of the wall of the supplementary vesicles is sufficient to enable their orifices of communication with a principal one to remain closed until their complete expansion takes place, or even to expand and collapse independently during the apparent obliteration of the principal vesicle, it appears to me that the view expressed by Mr. Carter in the 'Annals' for 1856 (vol. xviii. p. 129), namely, that "the sinuses

* From the extreme difficulty of determining whether we are looking at a contractile vesicle or a mere passive vacuole, I am unprepared to speak positively as to the behaviour of a detached segment when *apparently* devoid of the former of these organs—the diastolic condition being sometimes maintained without interruption for upwards of an hour.

of this system in the sarcode of *Amœba* not only seem to burst into *each other* and into the vesicula," is not only altogether irreconcilable with the facts advanced regarding the "complete isolation of the contractile vesicle and its supplementary cavities from the body and from each other," but irreconcilable with any other view than that the orifices of discharge are extemporized, and not permanent portions of the structure.

But we have some clue to the process by which the discharge of the contractile vesicle is supposed to be effected, according to Mr. Carter, from an observation made by him in the 'Annals' for 1856 (vol. xviii. p. 131), namely, that "in *Amœba* it [the contractile vesicle] is attached to the pellicula, and therefore no sarcode exists immediately opposite this point." Here, again, we find no mention of what is now described as taking place in *A. princeps*; for the remark is illustrated, not by any reference to that form, but to *A. radiosa*—no allusion being made to any fixed point of discharge or, indeed, any determinate aspects of the body, but it being simply stated that the figure appended "presents a mammilliform projection preparatory to discharging its contents."

Reverting now to the number of contractile vesicles, it will be seen that Mr. Carter expresses himself with perplexing ambiguity, as the subjoined extracts testify :—

"In *Amœba* and *Actinophrys* the vesicula is generally single; sometimes there are two, and not unfrequently in larger *Amœbæ* a greater number" ('Annals,' 2nd ser. vol. xviii. p. 128).

"There is no knowing how many vesiculæ there may be in *Amœba*; while *Actinophrys Sol* (Ehr.) is surrounded by a peripheral layer of vesicles, which, when fully dilated, appear to be all of the same size, to have the power of communicating with each other, and *each individually* to contract and discharge its contents externally as occasion may require; though, generally, one only appears and disappears in the same place" (*loc. cit.*, succeeding page).

"In *A. princeps* the normal number is one; but there are many smaller ones which *act* as sinuses around it, and one of these occasionally becomes so enlarged as to look like a second vesicula, yet it also ultimately discharges its contents into the main one. Where the vesicula discharges itself, it again recommences to appear; and there, also, the accessory sinuses may be best *seen* as they successively become dilated and discharge their contents into the vesicula" ('Annals,' July 1863, p. 38).

The condition of abnormal vacuolation referred to by me (in the 'Annals' for June, p. 436) as presaging disruption and death, is probably the same as that described by Mr. Carter as "an intense vacuolar state of the sarcode, which makes it look like an areolar tissue composed of vesicles, diminishing to a smallness that cannot be determined by the microscope." But he adds, "whether this state be a part of the vesicular system, or not, I am unable to decide." And it would appear that a similar opinion was held by him in 1856, from the subjoined statement extracted from the 'Annals,' vol. xvii. p. 358. "In *Amœba*, sometimes, the sarcode appears to be filled with such vesicles *, which not only now and then *burst* into the large one or *vesicula*, but, when the latter has discharged itself, frequently burst of themselves externally."

Without dwelling on the perplexing modifications of opinion, regarding the number of the contractile vesicles in *Amœba* and *Actinophrys*, which are embodied in the above extracts, I may observe that I regard the origin of the abnormal vacuolation as totally distinct from that of the multiple or supplementary contractile vesicles; and, bearing in mind that in *Amœba* it is connected with an exhausted condition of the organism, it appears explicable on the supposition that the effete watery particles, being unable to obtain a discharge through the ordinary endosmotic transference to the true excretory organ (namely the contractile vesicle, which now acts very sluggishly), are poured out, and produce vacuoles at any portion of the endosarc where a rudiment exists (see *antè*, p. 146). Should this view be correct, it would appear that the endogenously formed vacuoles constitute a rudimentary water-respiratory system †; whilst the contractile vesicle serves to throw off such portions of the watery particles as are effete; and the food-vacuoles (which are invariably formed at the surface) *ipso facto* constitute digestive cavities, whose assimilative function is called into action by the stimulus of organic objects capable of solution by them. In this sense I fully acquiesce in Mr. Carter's opinion that a digestive power is essentially inherent in sarcode *generally*, although I can no more admit the conversion of ectosarc and endosarc to be the result of a digestive process, as urged by him ('Annals,' July, p. 37), than that the absorption of a morbid growth, or the constant decay and renewal of parts, in the case of the higher animals is similarly brought about.

The conversion of endosarc into ectosarc I regard as analogous in its character, if not identical, with coagulation, the effect being produced by the mere contact of sarcode with the medium in which it resides; whilst the converse process constitutes an inherent vital function of the animal protoplasm. Should this view be admissible, we have presented to us a phenomenon bearing, in the most important manner, on the general question of development, and one which, I venture to affirm, is far more

* The context shows that the supplementary contractile vesicles are here referred to.

† The Diatomaceæ and Desmidiaceæ, when becoming languid and unhealthy, present this inordinate vacuolation.

largely engaged in the production of specific type, not only amongst the lower, but also the higher orders of being, than we have heretofore been inclined to allow. I allude to the reciprocal action of physical and vital forces.

Keeping in view, then, the proofs that have been adduced by me to show, 1st, that no permanent or determinate aperture exists either in the contractile vesicle, the supplementary vesicles, or in the outer layer (by whatever name called) of the villous appendage of *Amœba*; 2ndly, that, whilst the ectosarc is but a more consolidated condition of the endosarc, both endosarc and ectosarc are reciprocally convertible one into the other; 3rdly, that no appreciable difference is traceable between the ectosarc of the organism and the wall of the contractile vesicle when seen in its distended state; 4thly, that the coalescence of two distinct contractile vesicles takes place without reference to the special aspects in which they come into contact; 5thly, that no vestige of a permanent system of sinuses is discoverable, and that the facts actually observed militate in a direct manner against the possibility of its existence; 6thly, that the non-coalescence of a contractile vesicle with an ordinary vacuole, when coupled with what has been advanced under heads 2 and 3, and the fact that the obliteration of the extemporized aperture of the contractile vesicle takes place only when it comes into immediate contact with the ectosarc of the villous region, renders it extremely probable, if not certain, that the constitution of the wall of the one is identical with the investing layer of the other,—it appears to me to have been conclusively established that no determinate or permanent orifice occurs either in the villous region or the wall of the contractile organ *.

If, then, no permanent orifice exists at any portion of the wall of the contractile vesicle, and yet, notwithstanding, two or more of these organs have the faculty of coalescing, so as to constitute one vesicle, even after being so far removed from each other, and so subjected to change of relative position as to preclude the possibility of any bond of union such as a sinus being present,—it is manifest that we can only regard the coalescence of two or more vesicles as due to the gradual attenuation and ultimate disruption of the wall that intervenes between them. The appearances are those that would ensue from this process, and not such as would be likely to follow on an interchange of the con-

* On reference to the 'Annals' for June 1863, p. 441, it will be seen that I allude to the illusory appearance of an aperture in the contractile vesicle, engendered by an imperfect systole of that organ. I am still of opinion that this appearance is illusory, and shall reserve my views on the precise mode in which the discharge of the contents of the vesicle is brought about for a future occasion.

tents of two or more vesicles through a minute duct or aperture. In short, the process is identical with that observable on the coalescence of two adjacent soap-bubbles.

But it has been shown, I think, satisfactorily, both on evidence adduced in the preceding pages and from the opinion expressed by Dr. Carpenter (p. 138, *antè*)—namely, that "the contractile vesicle may be regarded as a vacuole with a defined wall,"—that the said wall is not identical in its degree of differentiation with the wall of the ordinary vacuolar cavities. The fact, already alluded to, of the contractile organ never coalescing with the true vacuoles would seem at once to establish this differentiation. Now it is not membranous in the usual acceptation of the term; but the appearance presented by its margin, its behaviour when isolated from the body altogether, as spoken of by Mr. Carter ('Annals,' July 1863, p. 39), and, since the publication of Mr. Carter's paper, verified by myself (with the exception of the iodine test), clearly prove that the differentiation in question is identical both in degree and character with that of the ectosarc generally. It is true that Mr. Carter (*loc. cit.*) refers to "the presence of condensed sarcode round the point of contraction manifested under the effect of iodine;" but this condensation is quite manifest without the iodine; and were it not so, I am inclined to think, as already urged, that the appearances presented *after* amorphous structure (such as that under notice) has been subjected to the action of a powerful chemical reagent are no guarantee that those appearances existed normally and prior to its employment. The condensed layer, moreover, may be seen whilst the contractile vesicle is still within the parent endosarc; and should it be isolated whilst in a state of contraction, the true villous character of the condensed layer becomes so palpable, that, but for the previous knowledge of its origin, it might readily be mistaken for a fragment of the villous appendage itself.

Mr. Carter's remarks on this head have such a material bearing on the view I put forward, that it is necessary for me to quote them in detail:—"Towards death, the vesicula, growing weak, is not easily refilled, nor do the small sinuses which surround it readily discharge their contents into it; so that by a little pressure, when the group is at the margin, they may be made to pass out into the water without bursting; and, at this time, if iodine be applied, each may be seen to retain its cell-form, puckered and tinted yellow by the iodine, *although they may be all quite isolated and separated from the rest of the sarcode and from each other*" (see figures, *loc. cit.*). Mr. Carter then asks, "If the vesicula be distinct, why not the sinuses?" (p. 39 *ut suprà*).

So far from admitting that Mr. Carter's view as to the permanent nature of the channel of communication between two or more supplementary vesicles (the analogues of the sinus-system of *Paramecium*, &c., according to that author), between the supplementary vesicles and the primary contractile vesicle, or between the principal one and the exterior, are borne out by the facts he thus describes and their illustrative figures, it appears to me that no facts could more directly negative the conclusions at which he has arrived,—in the first place, from the circumstance of "the small sinuses which surround" the primary vesicle being at all capable of isolation "from the rest of the sarcode and from each other;" and in the second, because the effect of iodine being to cause sarcode to contract and become consolidated, unless it can be shown that, besides mere reduction in bulk, such an increase of contractile power is secured as would prevent a determinate orifice from yielding under the tension to which the wall of the vesicle is subject, the retention of the cell-form, at the same time that the connecting sinuses are destroyed, is only reconcilable with one supposition, namely, that every portion of the vesicular wall is of uniform and unbroken composition. For I must repeat that since the changes of position usually undergone by every detached supplementary vesicle are as fortuitous as the shape of the body or the size of the pseudopodia, the difficulty of conceiving that these vesicles should revert to the precise point at which the excretory aperture is assumed to exist, so as to ensure that exact coincidence between the latter and their own excretory orifices which is essential to the stability of Mr. Carter's theory, must be regarded as insuperable.

I must also call attention to the difficulty of comprehending in what manner the prehensile power of the villi is effected, if the pellicula, which Mr. Carter declares to have no prehensile power ('Annals,' July 1863, p. 32), save when exercised under the "instinct" of the creature, invests the villous organ. It is clear that Mr. Carter assumes that it does so; otherwise he would not have made use of the expression, that there is an "aperture through the diaphane and pellicula" at that particular portion of the body.

Lastly, without offering any opinion on the question of "instinct," as here introduced, I have no hesitation in saying that the prehensile action observable in the villi of *Amœba villosa* is not of a grasping kind, as if they were minute pseudopodia, but distinctly adhesive and residing at the immediate surface. As stated by me ('Annals,' April, p. 288), so powerful is the prehensile action, that at times the villi become stretched beyond their endurance when the animal is moving. When this takes place to an inordinate degree, they are rent asunder, the torn extremity next the body starting back, at the instant of rupture, as if resilient.

Taking into consideration, then, the various facts that have been adduced on the subject in the present and preceding papers—that the characters of *A. princeps*, as assigned to it by Ehrenberg and Dujardin, have been universally accepted by writers on the Rhizopods up to the period at which my observations on *A. villosa* were published—the strong evidence afforded that *A. princeps* (Carter) is not a distinct form, but, together with other varieties to which separate specific names have heretofore been assigned, referable to *A. villosa*—that the characters of *A. villosa* are such as to elevate the genus to which it belongs considerably beyond the position it formerly occupied—and, lastly, that no descriptive notice or figures of any of the characters brought to notice in *A. villosa* had previously appeared in any printed work whatever,—I think it will be admitted that *A. princeps* (Ehr.), if still recognized at all as a species, should be retained under the definition originally assigned by its founder, whilst *A. villosa* should henceforth constitute the true type of *Amœban* structure.

I would state, in conclusion, that the length to which my observations have unavoidably extended, coupled with the absolute necessity for verbatim extracts, have precluded me from referring, in many cases, to the works of Ehrenberg, Dujardin, Schultze, J. Müller, Cohn, Lachmann, Claparède, Reichert, and others, and likewise from touching on numerous minor points bearing on the questions at issue. These omissions I hope hereafter to rectify. Meanwhile let me claim the reader's indulgence if I have been somewhat prolix in my treatment of a very important and imperfectly understood subject. In sustaining the accuracy of the opinions and statements published in my preceding papers, I had two distinct objects in view, namely, to advance science, and perform an act of justice to myself: for a very cursory perusal of Mr. Carter's notice on *Amœba princeps* will suffice to show that, directly or indirectly, nearly every opinion and statement of mine has been therein assailed.

Under these circumstances, should I have appeared somewhat tenacious of the little fame attaching to good service, I trust it may be taken into consideration that such service is not heaven-born, but the fruit of long and assiduous study, and that, however widely my friend Mr. Carter's views and mine may differ on certain points, we assuredly have no sympathy with those intellectual eagles who, whilst they affect to see everything at a glance, deny all credit to others, and would have the world believe that their aims are purely unselfish.

Kensington,
July 15, 1863.